

the Board of Education, South Kensington. The two little books under notice have been written in the same spirit, and contain some sections from the previous volume, but the treatment is more elementary and many new exercises are given.

In Part i. the subjects dealt with belong to arithmetic, algebra and the mensuration of parallelograms, triangles and polygons. Prominence is given to contracted methods, use of decimals, and explanations of algebraical expressions. Scales, calipers, and other simple measuring instruments are described in the chapters dealing with mensuration, and their use is well exemplified. Part ii. is devoted to logarithms, the slide rule, mensuration of circles, ellipses and irregular plane figures, volumes and surfaces of solids, more difficult algebraic expressions than are given in Part i., and the graphic representation of varying quantities. Among noteworthy points in this part may be mentioned the clear account of uses to which a slide rule may be put, the descriptions of planimeters, and the ingenious uses made of squared paper in the section on graphic representation.

The books are full of exercises illustrating the applications to every-day problems of the principles described, and at the end of Part ii. a set of tables of logarithms and anti-logarithms is given, to enable the student to work out problems by logarithms when convenient. It would be too much to say that the books contain an ideal course of mathematics for technical students, but they may fairly claim to provide far more inspiring information and serviceable exercises than can usually be found in text-books designed for use in schools.

Exercises in Natural Philosophy, with Indications how to Answer them. By Prof. Magnus Maclean, D.Sc., F.R.S.E. Pp. x + 266. (London: Longmans, Green and Co., 1900.)

THE ability to deal with quantitative results is an essential qualification of a student of physical science. Laboratory work provides some material for the exercise of this faculty, but it is often necessary to use data obtained by others, and to work out problems other than those which are afforded by the student's own practical work. Dr. Maclean's book contains numerous exercises of this character, covering most of the subjects studied in courses of physical science, and many worked-out examples of typical cases suggesting methods of solution for those which follow. Wisely used, the book will provide teachers with useful exercises in mathematics applied to physics, and will make a convenient supplement to text-books in which such exercises are not given. Many text-books do contain questions upon the subjects dealt with, but even in these cases some good additional problems for solution could be selected from the book under notice.

Tables of useful data and physical constants are printed at the end of the volume.

Memoirs of the Countess Potocka. Edited by Casimir Strylenski. Authorised translation by Lionel Strachey. Pp. xxiv + 253. (New York: Doubleday and McClure Company, 1900.)

THESE memoirs cover the period from the third partition of Poland to the incorporation of what was left of that country with the Russian Empire. They deal with episodes—more or less romantic and interesting—in Countess Potocka's career, referring to journeys, Court balls, and Napoleon I., between 1812 and 1820. The authoress died, at the age of ninety-one, in Paris, where her brilliant salon held no insignificant place in the gilded pleasures of the Second Empire. There is little of interest to scientific readers in the memoirs; but one or two incidents referring to astrologers are amusing.

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LETTERS TO THE EDITOR.

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Inverse or "a posteriori" Probability

THE familiar formula of Inverse Probability may be stated as follows:—

Let the probabilities of a number of mutually exclusive causes or conditions $C_1 C_2 \dots C_r$ be $P_1 P_2 \dots P_r$ respectively, and the probabilities that if $C_1 C_2$, &c., are realised, an effect or result E will happen be $p_1 p_2 \dots p_r$ respectively; then if E happens, the probability that it happened as a result of C_r is

$$\frac{P_r p_r}{\sum P p}$$

The current proofs of this are unsatisfactory, more especially one based on a theorem of James Bernoulli; for even if the ordinary statements of the principle of this theorem were correct, which must be disputed, the argument by which it is applied to Inverse Probability is demonstrably erroneous.

In consequence of the difficulty felt about the usual proofs, there seems to be a tendency to drop the subject, as unsound, out of mathematical theory.

Now it would not be hard to show that there is no essential difference of principle between problems of Inverse Probability and those of ordinary Probability, and therefore it can hardly be doubted that the former should admit of as accurate mathematical treatment as the latter.

The following is offered as a proof which can claim the same rigour as the theorems of ordinary Probability, and illustrates the identity of principle in both kinds of Probability:—

Let A and B be contingencies which are not independent, then, by a known theorem

Prob. concurrence of A and B = Prob. $A \times$ Prob. of B if A happens,

Or, as it may be shortly expressed,

Prob. A with B = Prob. $A \times$ Prob. B if A .

Similarly

Prob. A with B = Prob. $B \times$ Prob. A if B .

\therefore Prob. $A \times$ Prob. B if A = Prob. $B \times$ Prob. A if B .

$$\therefore \text{Prob. } A \text{ if } B = \frac{\text{Prob. } A \times \text{Prob. } B \text{ if } A}{\text{Prob. } B};$$

and this is really our theorem. For put $A = C_r$ and $B = E$.

$$\therefore \text{Prob. } C_r \text{ if } E = \frac{\text{Prob. } C_r \times \text{Prob. } E \text{ if } C_r}{\text{Prob. } E}.$$

But Prob. $C_r = P_r$, Prob. E if $C_r = p_r$, and obviously Prob. $E = \sum P p$ by a known theorem.

$$\therefore \text{Prob. } C_r \text{ if } E = \frac{P_r p_r}{\sum P p}.$$

Another demonstration may be given which, though a little longer, is quite simple.

If the whole number of "equally likely" cases with reference to a given contingency E is b , and the number of these in favour of E is a , then the mathematical probability of E is, of course, $\frac{a}{b} = p$, suppose.

Considered as a fraction, $p = \frac{na}{nb}$, where n is any quantity whatever.

Suppose n an integer, as a fractional value does not here concern us. We may consider each of the original "equally likely" cases as including n "equally likely" sub-cases; and then we can interpret the fraction $\frac{na}{nb}$ as we interpreted $\frac{a}{b}$, and say that there are nb new cases equally likely, and of these na are in favour of E .

Obviously, if x is the total number of equally likely cases, the number in favour of the event or contingency is px . Again, if q is the probability that E happens if C happens, this means that q of the equally likely cases of C 's happening are in favour of

E; and so, of course, there must be a total number y of such cases such that gy is an integer.

In the problem before us let $P_1, P_2, \&c.$, be reduced so as to have a common denominator b , then $P_1b, P_2b, \&c.$, are integers.

Multiply in each fraction $P_1P_2, \&c.$, numerator and denominator by n , taking n such that $p_1P_1nb, p_2P_2nb, \&c.$, are integers. Put $nb = x$.

Then of x equally likely cases, $P_r x$ is the number favourable to C_r . And, as above, the number of these again favourable to E , is $p_r P_r x$, that is, the number favourable to E happening as result of C_r (or " C_r if E ") = $p_r P_r x$; and the total favourable to E is $\Sigma p_r P_r x$.

Now, if the event E happens, the total of possible cases, of which one or other must be the true one, is clearly $\Sigma p_r P_r x$, and by hypothesis none of these cases has any preference over the other, and all are "equally likely;" while the number of them favourable to E resulting from C_r is $p_r P_r x$. Therefore the probability if E happens that it happens from C_r is

$$\frac{p_r P_r x}{\Sigma p_r P_r x}.$$

It may be noticed that a proof of the theorem that if A and B are not independent, Prob. A with $B = \text{Prob. } A \times \text{Prob. } B$ if A , which is repeated in edition after edition of ordinary textbooks, and so seems to have passed muster, is, nevertheless, erroneous.

The formula is proved correctly for two independent events, thus:—

Let a be the number of cases in which the first event may happen, b the number of those in which it fails; a' the number in which the second may happen, and b' the number in which it fails, the cases for each event severally being supposed equally likely. Each of the $(a+b)$ cases may be associated with each of the $(a'+b')$; thus there are $(a+b)(a'+b')$ compound cases which are equally likely. In aa' of these both events happen; therefore the probability of both happening

$$= \frac{aa'}{(a+b)(a'+b')} = \frac{a}{a+b} \times \frac{a'}{a'+b'} = \text{Prob. first event} \times \text{Prob. second event}.$$

It is then added that the above proof may be applied to two dependent events, for we have only to suppose that a' is the number of ways in which after the first event has happened the second will follow, and b' the number of ways in which after the first event has happened the second will not follow. Now if this substitution be made in the above, the first step of the proof will be "each of the $(a+b)$ cases may be associated with each of the $(a'+b')$ cases; thus there are $(a+b)(a'+b')$ compound cases which are equally likely." But this is impossible. Each of the $(a'+b')$ cases is one in which the first event happens, and therefore none of them can be associated with any of the b cases, because these presuppose that the first event has not happened. The $(a'+b')$ cases, in fact, can only be associated with the a cases out of the $(a+b)$, and thus the total number of the compound cases intended is not $(a+b)(a'+b')$. A proof can easily be given on the lines already indicated.

If P_1, P_2, n , and b have the same meanings as before, the whole number of equally likely cases is nb , the part favourable to the first event is $P_1 nb$, and the part of these favourable to the second is $p_1 P_1 nb$ (as above shown), which is therefore the number favourable to the concurrence of the two events. The probability, therefore, of the concurrence is

$$\frac{p_1 P_1 nb}{nb} = p_1 P_1$$

Certain confusions which often arise in the statement and application of the mathematical theory of probability would be avoided if a clear idea were formed of what is exactly meant by the fraction which is said to represent the probability of an event.

A good statement of the ordinary account of it is given in Todhunter's Algebra: "If an event may happen in a ways and fail in b ways, and all these ways are equally likely to occur, the probability of its happening is $\frac{a}{a+b}$, and the probability of its failing is $\frac{b}{a+b}$. This may be regarded as a definition of the meaning of the word probability in mathematical works."

A definition must not assume and use the notion to be

defined. Here probability is defined through cases "equally likely to occur"; but "equally likely to occur" means equally probable, and so the definition assumes the very notion which causes difficulty, the notion of "probability" or likelihood, and of which we require the explanation.

The first thing to settle is the meaning of these "equally likely" cases. Is the equal likelihood a quality in things themselves, or is it something in our minds only? If it is a quality in things it can only mean equal possibility of occurrence or realisation. But if a number of cases, mutually exclusive as intended in the above definition, were in the nature of things equally possible, not one of them could happen. If the claim of any one of them in reality were satisfied, so must the claim of any other, since these claims are equal, and therefore if one happens all must, but by hypothesis if one happens no other can; thus the only possible alternative is that none of them can happen. (It is precisely on this principle that we decide that the resultant of two equal forces at a point, whose directions include an angle, cannot be in any other direction than the bisector of the angle, and that there can be no resultant of two equal forces which act in opposite directions).

The equal likelihood then intended cannot be anything in the nature of things because it is assumed that one of the equally likely cases will happen. It is really only in our minds, when there is an equal balance of reasons for and against two or more events, and due solely to our ignorance, since if we knew which was to happen there could be no such balance and indecision. This is clear if we consider what is the reason why we pronounce one event more likely or probable than another; it is because we think there is more evidence in favour of the one than in favour of the other, however the "more" may happen to be measured. Two events are equally likely to us when we know nothing more in favour of the one than we do of the other—when the state of our knowledge and (it is important to add) of our ignorance, is the same for both contingencies. This view agrees with the actual procedure in mathematical examples. If a bag contains n balls, and one is to be drawn "at random," there are said to be n equally likely cases, that is, each of the n balls is equally likely to be drawn. Clearly this only means that as we don't know how the hand is going into the bag, we have no information in favour of the drawing of any one ball as compared with any other, and no information against the drawing of any one ball as compared with any other.

"Equally likely" cases then being such that owing to our ignorance the evidence in favour of any one is no greater or less than the evidence in favour of any other, the meaning of the definition of probability above criticised is evident; it is not a definition of probability, but it is the definition of a certain way of measuring evidence.

We are entitled to say that one event is more probable than another when the evidence before us, being decisive for neither, that in favour of the one seems to us, according to some standard of measurement, greater than the evidence for the other. Now what the mathematical analysis does is not to alter the ordinary meaning of "probability" at all, but to find a standard for the measurement of the more and less in evidence.

The whole possibility before us in any given contingency is divided into a number of cases, "equally likely" or "equally possible," in the sense that they are equal from the point of view of the evidence in favour of each of them; then if one event has more of these equal possibilities in its favour than another, it has in this sense "more" evidence in its favour, and so in accordance with the usual meaning (as above described) of "more probable," is more probable than the other. And here the "more or less" in the evidence is not a mere "more or less," but has a definite numerical measure. The evidence being, so to speak, divided into equal units, the strength of the evidence in favour of a contingency is measured by the number of these units in its favour. Thus if the total of equal possibilities, one of which must happen, for the events A, B and C is n , of which a involve A , b involve B and c involve C , the comparative strength of the evidence in favour of A, B and C respectively is measured by the ratios $a:b:c$, while the strength of the evidence of A, B and C respectively, as compared with the evidence for one or other of them happening (which is certain), is, on the same principle, measured by the ratios $\frac{a}{n}, \frac{b}{n}$ and $\frac{c}{n}$.

If, then, we symbolise the strength of the evidence for A, B and C by $\frac{a}{n}, \frac{b}{n}$, and $\frac{c}{n}$, and similarly that for one or other

of them happening by $\frac{n}{n} = 1$, these quantities have to one another the ratios required. We then arrive at the true meaning of the fraction which is said in mathematics to be the "probability" of a contingency; and much confusion might be avoided if we called the fraction, not the "probability," but the "*modulus of the evidence*," and the so-called equally likely cases not "equally likely" but "*equi-evidential*," or by some more convenient name conveying the same idea.

But it must be insisted that the above is only one way of measuring the evidence, and is not applicable to all cases. Indeed, the more important matters of daily life usually do not admit of it, for there are qualitative differences in strength of evidence which cannot really be measured quantitatively, and that is why the application of mathematical probability to the testimony of witnesses is so obviously futile.

The solution of every mathematical problem in probability is in the last resort only the finding of a modulus of evidence, in the ratio of the part of the whole number of equi-evidential cases which involve a given contingency, to the whole number of such cases; and with the finding of the modulus the strictly mathematical work ends. Mathematics, as such, has nothing to do with the inclination in our minds to expect the event for which the modulus of evidence is greatest (or "the probability" greatest), or the inclination, when some practical step has to be taken, to act on the hypothesis that the event will happen for which the evidence to us seems strong.

Unfortunately, however, there is too often a tendency to confuse the mathematical measure of the mere state of our minds with the measure of something in reality; and this produces various mistakes—e.g. the inclination to expect that the actual proportion of the occurrences of the event will tend to conform to the proportion represented by the mathematical probability, i.e. conform to a formula of our ignorance. This is an insidious fallacy, and we are not unlikely to fall into it in one form when we have escaped it in another; the mistake of supposing the mathematical probability could be confirmed by actual observation belongs to the head. The attempt to regulate betting by mathematical probability is another instance of the fallacy of confusing the subjective with the objective. The truth is that an observed average may be made the basis of a mathematical "probability" or modulus of evidence, by a process which could easily be explained; but though a "probability" may be based on an average, an average can never be based on a "probability."

J. COOK WILSON.

Instruments of Precision at the Paris Exhibition.

I WAS glad to see your appreciative article upon the German instruments of precision at the Paris Exhibition, in which you refer, among other things, to the splendid catalogue which was freely given away to any one who showed any interest and desired to have a copy.

As a member of the Jury of Class 15, I naturally was led to duly appreciate both the German productions and their catalogue, and fearing that this valuable record might too soon become inaccessible, I asked Dr. Drosten if he would send a copy to the Science Library of the Victoria and Albert Museum, so that it might be permanently available for many who might wish to see it. This he most willingly did.

If copies are becoming scarce, it would be more to the point that public libraries attached to scientific institutions should have them than that they should run the risk of being buried and lost in private hands.

C. V. BOYS.

A New Form of Coherer.

DURING the past eighteen months I have been called upon to demonstrate the principles of wireless telegraphy in connection with my regular lecture courses, and now and then, while wireless telegraphy was still the latest scientific novelty, in popular lectures.

For the latter purpose it was necessary to have the receiving apparatus as simple as was possible, compatible with a moderate sensibility and regularity of action.

I found the Marconi arrangement, consisting of the separate instruments, coherer, relay and decohering devices, to have the disadvantage, for my purpose at least, of requiring long and careful adjustment each time the apparatus was set up.

It occurred to me that if the functions of the three instru-

ments could be performed by a single instrument, an easier adjustment would result.

This would, perhaps, be of no advantage in the case of a permanent set up, but would be of considerable advantage in apparatus designed for the purpose of demonstration.

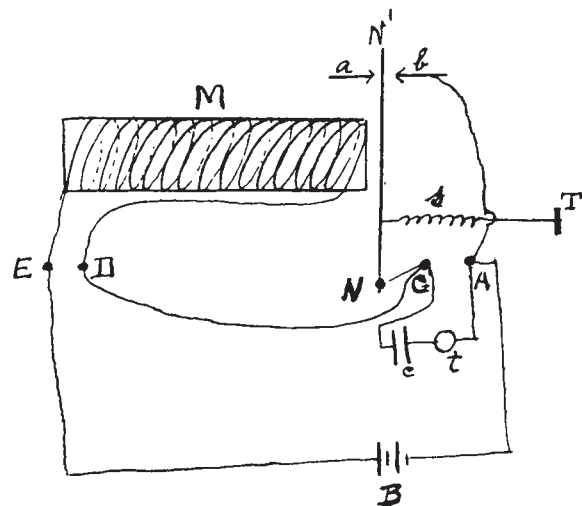
By a slight modification, which need not be permanent, an ordinary telegraph relay of moderate sensibility may be made to serve the purpose of the coherer, relay and decoherer of the Marconi arrangement.

The ordinary telegraph relay is shown in the accompanying sketch.

M is the electromagnet, which in most cases is mounted so that its distance from the armature, NN', can be varied by a slow-motion screw, E and D are the main circuit terminals, A and C the terminals of the relay circuit, C is connected with the armature NN', and A is connected to the stop a when the instrument is used as a relay. T is a screw connected to NN' by a spiral by means of which the pressure of the armature on the stop b may be varied.

Usually the stop b is of hard rubber, and a and b may be interchanged. If this interchange is made and if C is connected with D, then the battery B will send a current through the electromagnet M and the loose contact N'b.

The tension in the spiral, s, and the position of the electromagnet may be adjusted so that no current flows, on account of the very high resistance of the loose contact (coherer) N'b. If this resistance is lowered by electromagnetic radiations, then the



current through the electromagnet rises and NN' is attracted towards M and the circuit at N'b is broken. The spiral s draws NN' back into contact with b and the instrument is ready to again respond to electromagnetic radiations. The adjustment of M and T are easily made, and once made the coherer works very steadily.

The motion of NN' is too slight to be visible or to close an auxiliary circuit with a sounder, but if a telephone, t, in series with a condenser, c, is put in parallel with the coherer (i.e. across A C) the make and break of N'b are clearly audible.

If a "loud-speaking" telephone or a telephone with a manometric flame are used, the make and break can be made audible or visible to an audience.

If the distance between sending and receiving stations should make it necessary, C can be earthed and A connected with a vertical wire. It is well to have the resistance of the electromagnet as low as is compatible with moderate sensibility in order that the normal high resistance of the coherer shall form the major part of the total resistance in series with the battery.

In adjusting the contact N'b it is convenient to set M and T so that the armature NN' vibrates automatically, and then relieve the tension in the spiral s until the automatic vibration just ceases.

When this adjustment is made, a "dot" signal from the sending station gives a single "tick" in the telephone—a dash gives a series of ticks.

I have never attempted to telegraph over a distance exceeding